MOLECULAR THEORIES AND MATHEMATICS[1]

HOW could I fail to call up the memory of the illustrious scientist for whose death, so cruelly premature, France and the whole world are mourning? When Henri Poincaré was invited by President Edgar Odell Lovett to deliver an address at this scientific celebration, his acceptance was conditional on the state of his health. A few months later, he finally declined the invitation, promising, however, to send his lecture in writing. I cannot remember without emotion the last conversation I had with him on that subject. I was still hoping that his decision was not final; but, after giving me some friendly advice about my lectures and the journey, he told me with what deep regret he had to give up the thought of ever visiting the United States again, and I felt, for the first time, how serious was the condition which justified his refusal. A few weeks afterward he was gone. In spite of the difficulties of such a task, I should have considered it a pious duty to devote this address to an appreciation of his work; no subject could be more suitable, in this Institute consecrated to science, than the life and works of this noble champion of disinterested research; but my eminent friend Mr. Vito Volterra had, as you know, formed the same plan; and no one among you will regret that I resigned to him the privilege of carrying it out.

[1] An address delivered at the inauguration of the Rice Institute, by Emile Borel, Professor of the Theory of Functions in the University of Paris. Translated from the French by Professor Albert Léon Guérard of the Rice Institute.

I

THE relations between the mathematical sciences and the physical sciences are as old as these sciences themselves; it is the study of natural phenomena which led man to set for himself the first problems, out of which, by means of abstraction and generalization, the sciences of numbers and of space have grown in all their splendid complexity. Conversely, through a sort of preëstablished harmony, certain mathematical theories, after being developed apparently far from the real, were often found to provide the key to phenomena which the creators of these theories did not have in mind. The most famous instance in point is the theory of conic sections, an object of pure speculation among the Greek geometers, but whose researches enabled Kepler, twenty centuries later, to formulate with precision the laws of the motions of the planets. In the same way, in the first half of the nineteenth century, it was the theory of imaginary exponentials which made it possible to go deeper into the study of vibratory motions, which was found to be of such commanding importance in physics and even in the field of industry; it is to this study that we owe wireless telegraphy and the transmission of energy by polyphase currents. More recently still, we know how useful the abstract theory of groups proved to be for the study of the ideas, so profound and so new, which have been put forward to explain the results of the capital experiments on relativity made by your illustrious compatriot Michelson.

But these illustrations, however important they may be, are special and relate to particular theories. How much more striking is the universal adoption of the forms imposed on scientific thought by the genius of Descartes, Newton, Leibnitz! The use of rectangular coördinates and of the elements of differential and integral calculus has become so familiar to us that we might be tempted at times to forget that these admirable instruments date only from the seventeenth century, and in the same way the theory of partial differential equations dates only from the eighteenth century: it was in 1767 that d'Alembert obtained the general integral of the equation of vibrating chords. It was the study of physical phenomena which suggested the notions of continuity, derivative, integral, differential equation, vector, and the calculus of vectors, and these notions, by a just return, have become part of the scientific equipment necessary to every physicist: it is through them that he interprets the results of his experiments. There is evidently nothing mysterious in the fact that mathematical theories constructed on the model of certain phenomena should have been capable of being developed and of providing a model for other phenomena; this fact, however, deserves to hold our attention, for it implies an important practical consequence; if new physical phenomena suggest new mathematical models, mathematicians will have to study these new models and their generalizations, with the legitimate hope that the new mathematical theories thus evolved will prove fruitful in their turn in providing the physicists with useful forms of thought. In other words, to the evolution of physics there should correspond an evolution of mathematics which, without giving up the study of classical and well established theories, should develop in taking into account the results of experience. It is in this order of ideas that I should like to examine to-day the influence which molecular theories may have on the development of mathematics.

II

It was in the hypothesis of the continuity of matter that, at the end of the eighteenth century and in the first half of the nineteenth, what may be termed classical mathematical physics was created; one may take as types of the theories thus constructed hydrodynamics and elasticity. In hydrodynamics every liquid was considered by definition as homogeneous and isotropic; it was not quite the same in the study of the elasticity of solid bodies: the theory of crystalline forms had led physicists to admit the existence of a periodic network—that is to say, of a discontinuous structure; but the period of the network was supposed to be extremely small compared with the elements of matter physically considered as differential elements; the crystalline structure therefore led only to anisotropy, but not to discontinuity; the partial differential equations of elasticity as well as those of hydrodynamics imply that the medium studied is continuous.

Yet the atomic hypothesis, the tradition of which goes back to the Greek philosophers, was not abandoned; apart from the confirmation which it found in the properties of gases and in the laws of chemistry, it was by means of that hypothesis that certain phenomena, such as the compressibility of liquids or the permeability of solids, had to be explained, in spite of the apparent continuity of these two states of matter; but this hypothesis was placed in juxtaposition with the physical theories based on continuity: it did not affect them. The rapid advances in thermodynamics and in the theories of energy contributed to maintain this sort of impenetrable partition between the physical theories and the hypothesis of the existence of atoms, however fruitful this might prove to be in chemistry. For most of the physicists of half a century ago the problem of the reality of atoms was a metaphysical question, in the original acceptance of the term, a question beyond the domain of physics; it mattered little to science whether atoms existed or were simple fictions, and one might even doubt whether there was any sense in affirming or denying their existence. However, thanks especially to the labors of Maxwell and of Boltzmann, the explicit introduction of molecules into the theory of gases and solutions was proving its fruitfulness; and Gibbs created the new study to which he gave the name Statistical Mechanics. But it is only within the last twenty years that all physicists have been compelled, by the study of new radiations on the one hand, and by the study of the Brownian movement on the other, to consider the molecular hypothesis as indispensable to natural philosophy. And more recently a more thorough study of the laws of radiation has led to the unexpected hypothesis of the discontinuity of energy, or of motion. It does not come within my subject to expound the experimental proofs which make these hypotheses seem more and more probable every day; the most striking experiments are perhaps those which have made it possible to observe the individual emissions of the α particles, so that we are actually able to apprehend one of the concrete units with which the physicist builds up the sensible universe, just as the abstract universe of mathematics can be built up by means of an abstract unit.

In order definitely to formulate their hypotheses and to deduce therefrom consequences that can be experimentally verified, the theorists of modern physics make use of mathematical symbols; these symbols are those which were created on the basis of the notion of continuity; no wonder, therefore, if difficulties sometimes appear, the most recent of which is the contradiction, at least in appearance, between the hypothesis of the *quanta* and the older hypothesis that phenomena are governed by differential equations. But these difficulties of principle do not prevent the success of what may be called partial theories, by which a certain number of experimental results, in spite of their apparent diversity, can be deduced from a small number of formulæ which are coherent among themselves; thus, for many of the phenomena of physical optics, the formulæ are the same in the mechanical theory of Fresnel and in the electromagnetic theory of Maxwell; in the same way, the formulæ used by electrical engineers are independent of the diversity of theories concerning the nature of the current.

If I have made it a point to call your attention to this use of the mathematical instrument as an auxiliary to the partial physical theories, although it does not lie within my subject, it is in order to prevent any misunderstanding: it seems to me beyond doubt that for a long time to come—perhaps as long as human science itself shall endure—it will be under this comparatively modest form that mathematics will prove of greatest use to physicists. This is no reason why we should take no interest in the general mathematical theories for which physics has provided the models, whether we have to deal with speculations on partial differential equations suggested by the physics of the *continuum*, or with statistical speculations pertaining to the physics of the *discontinuum;* but it should be clearly understood that the new mathematical theories which may be suggested by the discontinuity of physical phenomena cannot have the pretension of entirely replacing classical mathematics; these are only new aspects, for which it is proper to make room by the side of the older views, so as to increase as much as possible the richness of the abstract world, wherein we seek for models which will make us understand concrete phenomena better and foresee them more accurately.

III

It is frequently a simplification in mathematics to replace a very large finite number by infinity. Thus the calculus of definite integrals is frequently more simple than that of summation formulæ, and the differential calculus is usually simpler than that of finite differences. In the same way, we have been led to replace the simultaneous study of a very large number of functions of one variable by the study of a continuous infinitude of functions of one variable; that is to say, by the study of a function of two variables. By a bolder generalization, Professor Vito Volterra has been led to define functions which depend on other functions—that is to say, in the simplest case, functions of lines, in considering them as the limiting cases of functions which would depend on a great number of variables, or, if one prefers, on a very great number of points of the line.

These various generalizations have rapidly secured permanent recognition in mathematical physics; the use of integral equations, the classical types of which are the equation of Volterra and the equation of Fredholm, has become current. Although these theories are well known to all, it may not be idle to recall their origin by means of a particularly simple example; we shall thus better understand their significance from our present point of view.

Let us consider a system composed of a finite number of material points, each of which can deviate only by a small amount from a certain position of stable equilibrium. The differential equations which determine the variations of these deviations from their position of equilibrium may be considered, under certain hypotheses and to a first approximation, as linear in respect to these deviations. If, moreover, we introduce the hypothesis that the system conforms to the law of the conservation of energy, the differential equations assume a very simple and classical form, from which the fact can easily be deduced that the motion may be considered as the superposition of a certain number of periodic motions. The number of these elementary periodic motions is equal to the number of degrees of freedom; it is three times the number of the material points, if each of these points can be arbitrarily displaced in the neighborhood of its position of equilibrium. The periods of the simple periodic motions are the *specific constants* of the system, which depend only on its configuration and the hypotheses made concerning the forces brought into action by its deformation, but which do not depend on the initial conditions: positions and velocities. These initial conditions determine the arbitrary constants which figure in the general integral and which are two in number for each period: the intensity and the phase.

Now let us suppose that the number of material points becomes very large, and let us identify each of them with a molecule of a solid body—a bar of steel, for instance; if our hypotheses are still verified—and this is admitted in the theory of elasticity—their consequences also will remain true; we shall then have a very large number of characteristic constants, each of these constants defining a proper period of the system. Let us increase to infinity the number of molecules; the system of differential equations, infinitely great in number, is then replaced by a finite number of partial differential equations, whose fundamental properties are obtained by passing to the limit. In particular, the proper periods can be determined, and this remarkable result is established—that these periods can be calculated with precision and without ambiguity if we take the precaution of defining them by commencing with the longest; there is only a finite number of periods superior to a given interval, but this number increases indefinitely when the interval tends toward zero.

The reasoning which has just been outlined is the type of those to which the substitution of continuity for discontinuity leads; in reality, the considerations based on the existence of molecules play but an auxiliary part in them; they put us on the track of the solution, but this solution, once arrived at, satisfies rigorously the partial differential equations of Lamé, equations which can be deduced just as well from theories of energy as from molecular hypotheses. The molecular theory has therefore been a valuable guide for the analyst in suggesting to him the course to be followed in studying the equations of the problem, but it is eliminated from the final solution. On the other hand, we know that this solution is but an imperfect representation of reality; we obtain an infinitude of proper periods, instead of a very great number of them; the actual number, however, is so great that we ought not, perhaps, to feel any scruple in passing to the limit and considering it as practically infinite. If, however, one bears in mind that the difficulties of the theory of black radiation arise precisely from the very short periods, and that these difficulties are not yet solved in an entirely satisfactory manner, one will perhaps come to the conclusion that one could not be too careful about anything which relates to these very short periods. This is probably the reason why such a physicist as Lorentz has thought that the considerable analytical efforts required by the study of the propagation of waves, when molecules are explicitly introduced into it, were not superfluous. However this may be, even if the substitution of the infinite for the finite is entirely legitimate in certain problems, it may be interesting to propose to one's self, from a purely mathematical point of view, the direct study of functions or equations depending upon a great but finite number of variables.

IV

The first difficulty which presents itself, when one wishes to study functions of a very great number of variables, is the exact definition of such a function—I mean its *individual* definition—making it possible to distinguish the function thus defined from the infinitude of other analogous functions. It is true that there exist general properties common to all the mathematical entities of a certain category, independent of the numerical value of the coefficients; for instance, every definite quadratic form (that is to say, one always positive) is equal to the sum of the squares of as many independent linear functions as the number of the variables which it contains. One has at times attempted to deduce physical consequences from mathematical facts of that kind; I must confess that I cannot help being skeptical about this sort of reasoning; it may seem rather strange that one should be able to deduce anything exact from such a general notion as that of a surface of the second degree (let us say, for fixing ideas, a generalized ellipsoid) in a space having a very great number of dimensions. Let us insist a little on the difficulty there is in knowing such an ellipsoid *individually:* its equation may be supposed to be reduced to a sum of squares by an orthogonal substitution—that is to say, the axes remaining rectangular. Such an ellipsoid then requires, for its complete definition, the knowledge of what we may call the squares of the lengths of its axes—that is to say, the knowledge of as many positive numbers as the space considered has dimensions. The question of knowing whether one can consider as *given* so many numbers, when a man's lifetime would not suffice to enumerate a small part of them, is a question which is not without analogy with that of the legitimacy of certain reasonings of the theory of ensembles, such as the one by which Professor Zermelo pretends to prove that the continuum can be well ordered, and which supposes to be realized an infinitude of choices independent of any law, and yet uniquely determined. Opinions may differ on the theoretical solution of these difficulties, and this is not the moment to reopen this controversy; but from the practical point of view, the answer is not doubtful: it is not possible effectively to write the numerical equation of an ellipsoid whose axes are as numerous as the molecules constituting a gram of hydrogen.

In what sense then is it possible to speak of a numerically determined ellipsoid possessing a very large number of dimensions? From an abstract point of view, the simplest method for *defining* such an ellipsoid consists in supposing that the lengths of the axes are equal to the values of a certain function which is simple for the integral values of the variable; one may suppose them to be all equal (in which case one will say that the ellipsoid is reduced to a sphere); one may also suppose that their values are the successive integral numbers in their natural sequence, either starting from number one or from any other given number; or that they are equal to the inverses of the squares of these integers, etc. In other words, we suppose that the lengths of the axes are all determined by the knowledge of a formula simple enough to be actually written, whereas it is not possible actually to write as many distinct numbers as there are axes.

Another method, to which we are naturally led by the analogies with the kinetic theory of gases, consists in supposing that the values of a function of the axes, such as the square of the lengths of the axes, or of their inverses, etc., are not individually given, but that we know only the mean value of this function, and the law of the distribution of the other values around this mean. We propose, under these conditions, not to study the property of a unique and well defined ellipsoid, but only the most probable properties of the ellipsoid, knowing only that it satisfies the required conditions; we can also say that we study the mean properties of the ensemble of the ellipsoids defined by these conditions. Here again we may observe that the probable ellipsoid or the mean ellipsoid is completely defined by the knowledge of the mean value of the law of deviations. If this law is the classic law of probabilities, it includes only two constants; if we were led to introduce a more complicated law, this law might in all cases be explicitly written. The two processes that we have indicated are therefore equivalent from the analytical point of view; it would evidently be the same with all other processes that could be imagined, and in particular with the combinations of these two.

In a word, a figure which depends on an extremely great number of parameters can be considered as numerically determinate only if these parameters are defined by means of numerical data sufficiently few in number to be accessible to us. It is for this reason that the study of the geometrical figures in a space possessing an extremely great number of dimensions can lead to general laws if we can exclude from this study such of these figures as, humanly speaking, cannot possibly be defined individually.

Here are, for example, some of the results to which the study of ellipsoids leads us. In working the equation in the form of a sum of squares, the second member being reduced to unity, the coefficients are equal to the reciprocals of the squares of the axes. If the mean of the squares of these coefficients is of the same order of magnitude as the square of their mean, one will say that the ellipsoid is not very irregular. The modes of definition concerning which we have just spoken lead to ellipsoids which are not very irregular, since one does not systematically introduce into those definitions functions purposely chosen in a complicated manner. On the contrary, one gets a very irregular ellipsoid in equating to a constant the *vis viva* of a deformable system composed of a very great number of molecules, this *vis viva* being written under the classic form of the sum of the *vis viva* of translation of the total mass concentrated at the center of gravity, increased by the sum of the *vires vivæ* of the molecules in their motion relative to this center of gravity. The great irregularity comes from the fact that the products of the total mass by the three components of the velocity of the center of gravity are extremely great in comparison with the other terms. When an ellipsoid is not very irregular, several of its properties make it possible to assimilate it to a sphere, which may be called the median sphere; the surface of the ellipsoid is almost wholly comprised between the surfaces of two spheres very close to the median sphere; on the other hand, if a point is arbitrarily chosen on the ellipsoid, it is infinitely probable that the normal at this point passes extremely close to the center.

This geometrical study of figures with a very large number of dimensions deserves, I believe, to be thoroughly investigated; it brings out the abstract basis of the theories of statistical mechanics and physics—that is to say, it enables us to distinguish, among the propositions to which physicists are led, those which are a consequence of physical hypotheses from those which are derived only from statistical hypotheses. But, apart from its physical usefulness, this geometrical study of spaces having a very great number of dimensions offers an interest of its own; it is to the molecular theories that we are indebted for this new branch of mathematics.

V

We can, however, ask ourselves whether it is legitimate to consider as bound up with the molecular hypothesis a theory which, after all, should depend exclusively on a small number of constants. To say that an ellipsoid with a great number of dimensions is entirely defined by five or six constants, amounts to saying that all the consequences which we shall deduce from its study can be expressed by means of these five or six constants. Can we not suppose, then, that an analytical mechanism could be devised, enabling us to arrive at these same consequences, expressed by means of the five or six constants, without its being necessary to bring in the equation with a very great number of terms—that is to say, without its being necessary to make use of the molecular hypothesis.

This objection deserves careful consideration, although it reminds us of the controversy between the energetists and the atomists, a controversy in which the advantage seems decidedly to have been on the side of the atomists. It may be answered, in the first place, with an argument of fact: it matters little that we might conceive the possibility, without making use of molecular hypotheses, of combining among themselves the consequences of these hypotheses; the important point is to know whether this possibility is realized at present, or if, on the contrary, the calculations based upon molecular hypotheses are the simplest, if not the only, mode of deduction. If the latter alternative be correct, and it seems difficult to deny that it is, molecular hypotheses are therefore at present very necessary indeed, and that alone ought to be of consequence to us.

Under this modest form, which leaves room for future contingencies, this reply seems peremptory; but I believe that many physicists would think it is not categorical enough. It must be noted, however, that the question is independent of the experimental proofs of the reality of molecules. Even if we should succeed in seeing, by means of an instrument more powerful than a microscope, the molecules of a solid body, it would not follow, however valuable this knowledge might be, that one should have to use it in order to account, in the simplest possible manner, for the properties of that body; in a similar way, the possibility of seeing an isolated microbe under the microscope is not an indispensable condition for the attenuation of the viruses and the use of vaccines; or again, in the reproduction of a masterpiece by photogravure, it is not the individual knowledge of the points constituting the negative that interests us.[1]

From an abstract point of view, if we admit that any human theory must be expressed, in last analysis, by means of a finite and relatively small number of data, it seems difficult to deny the possibility of entirely constituting the theory, without introducing hypotheses which imply the existence of elements more numerous than human imagination can conceive. But the recognition of this abstract possibility cannot prevail against the importance of the services rendered by molecular theories in linking together apparently unrelated phenomena; so it is permissible to consider these reserves on future possibilities as purely theoretical.

[1] This individual knowledge of points has a part in the processes for transmitting the negative to a distance; but in this case these points, however numerous, are none the less finite in number and accessible to our observation. If we transmit by telephone an orchestral selection, we know that all the æsthetic beauties of the piece are, in last analysis, the results of certain vibrations which would require too much time to be known individually; but in fact these elementary vibrations have nothing to do with musical æsthetics: an excellent composer may be ignorant of their existence, and an excellent physicist may be a wretched musician.

Is it possible to go still further, and to do away even with this kind of reserve? In order to answer this question, we should have to examine in detail all the phenomena which are explained by means of molecular hypotheses, and to try to ascertain whether an extremely large number of parameters is indeed necessary to such explanation. Among the discontinuous phenomena whose experimental laws are well known, the most characteristic are those of spectra in series; we know that the positions of the spectral rays are determined with a very great precision by formulæ, the first and simplest of which, due to Balmer, includes the difference of the reciprocals of the squares of two integers. This is perhaps the most remarkable example of the intervention of the integer in natural law; if laws of this kind were more numerous and better known, one might possibly be led to name arithmetic and the theory of numbers among the branches of mathematics which can be connected with molecular physics. Can one, by induction, admit that the formula of Balmer is exact, not only for small integers concerning which the experimental verification is rigorous, but for many other larger integers concerning which this verification is impossible? And if such be the case, is it not one of those discontinuous phenomena whose explanation requires a very large number of parameters? It does not seem so: on the one hand, the formula with the variable integer contains in fact but a small number of constants; on the other hand, the attempts made for explaining the presence of this integer by hypotheses of physical discontinuity have led to the placing of this discontinuity within the atom itself; there is consequently no need of a very large number of atoms: one alone is sufficient, whose structure depends only on certain parameters, on *magnétons* in the theory of Ritz, parameters the number of which is far from being of the same order as the number of the atoms.

This remark leads us to consider another category of phenomena, to which we have already alluded, and in which the atoms or corpuscles are observed individually. Does not the explanation of these phenomena require atomic hypotheses? It seems difficult to deny it without being paradoxical. Let us note, however, that such phenomena as the emission of the α particles are susceptible only of a globate explanation; it is not possible to foresee with accuracy any particular emission, but only a mean number; scientifically speaking, therefore, this mean number alone has any existence; the phenomenon which consists in the emission of one α particle does not present the characters which permit of rigorous experimentation: one cannot either foresee it or reproduce it at will; it is only the study of the trajectory *after* the emission that offers these characters; and in fact this study requires only such a limited number of equations that one can write them all. The atomic hypotheses would enable us to foresee each individual emission, if one could in fact calculate with reference to an extremely great number of equations; but that is not possible, and so far as the *globate* prevision is concerned the atomic hypothesis is not, at least *a priori,* necessary.

We touch here upon the borders of science, since we reach phenomena accessible to our observation, and which depend upon causes too numerous for us ever to know them with precision in their full complexity. Science remains possible only for mean values which can be calculated with precision by means of data accessible to observation.

It is well understood, I hope, that I do not dispute the legitimacy and usefulness of molecular theories; my remarks as a mathematician cannot attain physical reality; at the bottom, they do not go farther than this: all the calculations we shall ever be able really to effect will comprise only a rather limited number of equations actually written; if we write one equation, and if we add that we consider several billions of analogous equations, we do not, in fact, calculate these unwritten equations, but only the written equation, taking into account perhaps the number of these unwritten equations, a number which also has been written. Every mathematical theory, therefore, reduces itself to a relatively small number of equations and calculations, which involve a relatively small number of symbols and numerical constants; it is therefore not absurd *a priori* to suppose that one might conceive a physical model containing also a relatively small number of parameters and leading to the same equations. As long, however, as this model has not been imagined—and perhaps it will never be—the analytical or geometrical researches on functions of a very large but finite number of variables will offer some interest for the physicists.

VI

WE have already observed that it is an ordinary proceeding in mathematics to replace a very large finite by an infinite. What result does this method yield when it is applied to physically discontinuous phenomena, whose complexity seems bound up with the very large number of molecules? Such, for instance, are the phenomena of the Brownian movement, which is observed when very fine particles are in suspension in an apparently quiet liquid. These phenomena fall within the category of those we were mentioning a moment ago, of which none but a statistical foreknowledge is possible.

Is it possible to construct an analytical image of such phenomena? Professor Jean Perrin[1] has already called attention to the fact that the trajectories observed in the Brownian

[1] Jean Perrin, "La discontinuité de la matière," *Revue du Mois*, mars 1906. See also Jean Perrin, "Les Atomes," Alcan 1913.

movement suggest the notion of continuous functions possessing no derivatives, or that of continuous curves possessing no tangent. If one observes these trajectories with optical instruments of increasing perfection, one sees, at each new magnification, new details, the curvilinear arc that we could have traced being replaced by a sort of broken line the sides of which form a finite angle with each other; this remains the case up to the limit of the magnifications at present possible. If we admit that the movement is produced by the impact of molecules against the particle, we must conclude that, with a sufficient magnifying power, we should obtain the exact form of trajectory, which would present itself under the form of a broken line with rounded angles, and which would not be perceptibly modified by a still further magnification.

But the analyst is not forbidden to put off indefinitely in his thought the realization of this final state, and thus to arrive at the conception of a curve in which the sinuosities become finer and finer as one uses a higher magnification, without ever obtaining the final sinuosities: this is indeed the geometrical image of a continuous function not admitting of a derivative.

We obtain also a curve of a similar nature, sufficiently interesting to arrest our attention, when we study the function which Boltzmann designates by H and Gibbs by η, and which represents, in the case of a gas, the logarithm of the probability of a determinate distribution of the velocities of the molecules. Each collision between two molecules gives a sudden variation to this function, which is thus represented by a staircase curve, the horizontal projections of the steps corresponding to the intervals of time which separate two collisions, the number of the collisions undergone by a molecule being some billions per second (that is to say, of the order of magnitude 10^9), and the number of molecules of the order of magnitude 10^{24} (if we consider a mass of a few grams of gas), the *total* number of collisions per second is of the order of magnitude 10^{33}; such is the number of steps projected on a portion of the axis of the abscissæ equal to unity, if the second is taken as the unit of time.[1] What the physicists consider is the mean behavior of the curve. They replace the serrated curve by a more regular curve having the same mean behavior in the time intervals, which are very small in comparison to the second, but very great in comparison to 10^{-33} of a second.

These diverse considerations bring interesting suggestions to the analyst, on which I should like to dwell for a moment.

In the first place, referring to the continuous curves without derivatives of which the Brownian movement has given us the image, should the passage from the finite to the infinite lead to a curve *all* of whose points are points of discontinuity, or to a curve which admits an infinitude of points of discontinuity, but also an infinitude of points of continuity? For a proper understanding of the question, it is necessary briefly to recall the capital distinction between denumerable infinity and continuous infinity. An infinite ensemble is said to be denumerable if its terms can be numbered by means of integers. Such is the case for the ensemble composed of terms of a simple or multiple series; we can also cite as a denumerable ensemble the ensemble of the rational numbers. On the other hand, the ensemble of all the numbers comprised between 0 and 1, both commensurable and incommensurable, is not denumerable: we say that this ensemble has the same power as the *continuum*. If we define a discontinuous func-

[1] This discontinuity supposes evidently that we consider the duration of a collision as less than the mean interval of two collisions (in the whole mass), a hypothesis difficult to admit. The *schema* to which this hypothesis leads is not less interesting from the analytical point of view.

tion by a series each term of which admits a point of discontinuity, the ensemble of these points of discontinuity is denumerable, as are the terms themselves. Can we determine a function which shall be totally discontinuous—that is to say, one whose points of discontinuity shall be all the points of a continuous ensemble, and not merely those of a denumerable ensemble? It would seem to be easy to imagine such a function. Such is the oft-studied function which is equal to 1 if x is commensurable and to x if x is incommensurable; this function is indeed discontinuous, as much so for the commensurable values as for the incommensurable values. If we look a little closer, we perceive that the discontinuity is not the same in these points: we must note, in fact, that the commensurable numbers occupy infinitely less space in the axis of the x's than do the incommensurable numbers; the ensemble of these commensurable numbers is of dimension zero—that is to say, it can be confined within intervals whose total extent is less than any number given in advance. Speaking in more concrete terms, if we choose a number at random, the probability that it will be commensurable is equal to zero.[1] We therefore conclude that the function equal to x for the incommensurable values of the variable is, *on an average,* continuous for these incommensurable values, whatever its values may be for the commensurable values—that is to say, if we choose in the neighborhood of an incommensurable value, for which we study the continuity, another value *taken at random,* it is infinitely probable that this value taken at random will also be incommensurable; it is then infinitely probable that the variation of the function will be infinitely small when the variation of the variable is small.

[1] To give one's self a number at random, one may agree to choose at random the successive figures of the decimal fraction which is equal to it; the probability that this decimal fraction will be finite or periodic is evidently equal to zero.

This remark enables us to understand that it has not been found possible to define analytically a function all the points of which are effectively points of total discontinuity; it is only in points determined according to the definition of the function, and playing a particular part in this definition, that the function is actually discontinuous on an average.

The passing from the finite to the infinite, when we are concerned with the discontinuity of functions, is, then, not effected after the manner which is most usual in classical mathematical physics, in which matter is supposed to be continuous, and in which the finite is replaced by the continuous; we are led to conceive a different process, which seems, besides, more in harmony with the molecular conception, and which consists in replacing the very great finite by the denumerable infinite.

This is the way in which the analytical generalization of such curves as the H curves presents itself from this point of view. Let us consider a number written in the form of an interminate decimal fraction, and let us imagine that the figures which follow the decimal point are grouped in successive periods, each period containing many more figures than the preceding period. To each period we shall cause to correspond one term of a series, this term being equal to zero if in the corresponding period the ratio of the number of even figures to the number of odd figures is comprised between 0.4 and 0.6; while if this ratio is not comprised between these limits, the term corresponding to the period is equal to the term of the same order of a certain convergent series with positive terms. It is clear that, if the lengths of the successive periods increase rapidly, it is infinitely probable that a small number of periods only will furnish terms different from zero; consequently, the series which corresponds to the decimal number will be terminate; this terminate series has a certain sum, which remains the same as long as the decimal number varies so little that the last one of the periods which gave a term to the series is not modified; at least in the interval thus defined it is extremely probable that the function corresponding to the decimal number preserves this constant and well determined value—that is to say, is represented by a horizontal line; however, there are in this interval, as in every interval, particular decimal numbers for which certain periods of high order, perhaps even an infinitude of such periods, are irregular from the point of view of the distribution of even and odd figures; there are then intervals which are extremely small, and, on an average, extremely rare, but nevertheless dense everywhere, in which the curve runs up above the horizontal line which in general represents it. In one of these points, which we may call maxima of the curve, it is extremely probable that, if we take a value in the neighborhood of the variable at random, the function will diminish—that is to say, that this point has, on an average, the character of a maximum in a point.

In the preceding example the maxima are represented by intervals narrower and narrower, but finite; in modifying slightly the definition, one can obtain a curve which would coincide everywhere with the axis of x, except in points not filling any interval; it is sufficient to agree that, in the series which we have just defined, we replace by zero every term which is followed by an infinitude of terms equal to zero; the new series can then be different from zero only if the terms of the first series are all, after a certain place, different from zero.

The study of analytical models thus obtained leads us to go deeper into the theory of functions of real variables, and even to conceive new notions such as the notion of *average derivative,* naturally suggested by the physical example of the function H.[1] Besides, it is necessary to observe that in the study of these functions the notion of continuous ensemble is often combined with the notion of denumerable ensemble; for example, it is easy to see that the ensemble of decimal numbers whose figures are all odd presents certain characters of the ensemble of all the decimal numbers; it has, as we say, the same power as the continuum,[2] but it is, however, of zero dimension.

We may also connect with these considerations the theory of denumerable probabilities—that is to say, the study of probabilities in the case in which either the infinitude of trials or the infinitude of possible cases is denumerable—a study lying between the study of probabilities in the finite cases and the study of continuous probabilities.

VII

In spite of the interest of problems relating to functions of a real variable, it is the theory of functions of a complex variable which, since the immortal discoveries of Cauchy, is really the center of analysis. The analogy between the theory of the functions which Cauchy has called monogenic functions and which are often called analytical functions, and the theory of Laplace's equation which is verified by potentials, is undoubtedly one of the most fruitful analogies in analysis. We know all the advantage Riemann has derived from the theory of potential and from physical intuition in his profound researches upon the functions of a complex variable.

[1] Emile Borel, "Comptes Rendus de l'Académie des Sciences de Paris," 29 avril 1912.

[2] If in a decimal number all of whose figures are odd we replace the respective figures 1, 3, 5, 7, 9 by the figures 0, 2, 3, 4, we may consider that number as any number whatever written in the system whose base is 5.

It is therefore natural to ask one's self what new ideas can be brought by molecular theories into this domain of complex variables. Here again we shall be led to replace the very large finite number by the denumerable infinity: it is easy to form series each term of which presents a singular point, the ensemble of the terms of the series thus possessing a denumerable infinitude of singular points. These singular points may, for instance, be so chosen that they coincide with all such points among the points inside of a square whose two coördinates are rational. The most simple series that we can thus form presents itself under the form of the sum of a series of fractions each of which admits of only one pole, which is a simple pole. The physical interpretation, in the domain of reality, of such a series leads us to consider the potential of a system composed of an infinitude of isolated points, the mass concentrated in each of these points being finite (which leads us to admit that the density in each such point is infinite, if the point is considered abstractly as a simple geometrical point without dimensions). We suppose, of course, that the series whose terms denote the values of the masses is convergent, which amounts to saying that the total mass is finite, although concentrated in an infinitude of distinct points—for example, in all the points whose two coördinates are rational numbers.

The potential with which we are now concerned is in the case of a plane what we call a logarithmic potential; we could reason similarly in three-dimensional space: we should then have the Newtonian potential properly so called.

The hypothesis that the attracting masses are simple material points without dimensions is difficult to accept from the physical point of view; one is thus led to perform the analytical operation which consists in dispersing this mass into a small circle (or a small sphere) having this point for center, without changing the potential outside of this circle (or sphere); we shall call this circle (or sphere) the "sphere of action" of the point which coincides with its center; we shall choose its radius to be proportional to the mass concentrated at its center, so that, if the series formed by the masses converges with sufficient rapidity, we may arrange things in such a manner that the radii of the spheres of action also form a very rapidly converging series, and yet that the maximum density of the attracting mass be finite. It is also easy, if we admit that we can dispose arbitrarily of the distribution of masses and densities, to arrange things in such a way that the distribution in each sphere of action, as well as its derivatives, is reduced to zero over the whole surface of the sphere; the distribution of the density is thus not merely finite, but continuous throughout space.

The hypothesis which we have made concerning the convergence of the series the terms of which are the radii of the spheres of action, implies the convergence of the series the terms of which are the projections of these spheres on any straight line whatever; if, therefore, in this series, we suppress a certain number of the first term, the rest of the series can be made less than any number fixed in advance. From this we conclude that in an interval, however small it may be, taken on the straight line on which we project the spheres, we can find an infinite number of points which belong at the most to a finite number of such projections—namely, those belonging to the spheres S which correspond to the first terms of the series, and which were suppressed in the series in order to make the remainder less than the interval considered. If we consider a plane perpendicular to the straight line and passing through one of these points (this point being chosen, as is possible, distinct from the projections of the centers of the spheres S, finite in number, concerning which we have just spoken), this plane will at most intersect a finite number of spheres S, without going through their centers, but will be exterior to all the other spheres of action. It is possible to modify the distribution of matter within the spheres S which are finite in number and intersected by the plane in such a manner as to replace these spheres by smaller ones which do not intersect the plane, this operation not modifying the potential outside of the spheres, and the density remaining finite, since the operation relates to only a limited number of spheres. To sum up, it is possible to find a plane perpendicular to any straight line whatever, cutting out of this line any segment whatever given in advance, and such that in all the points of this plane the density shall be zero. Since our potential function is defined by a density everywhere finite and continuous, this potential satisfies the equation of Poisson, which reduces itself to the equation of Laplace wherever the density is zero—that is to say, in all the points of the planes which we have just defined. It was not idle to insist upon this point, for these planes may traverse regions of space in which the given material points are everywhere dense—as are, for example, all the points whose coördinates are rational numbers. We might have feared that there would be no free space between points so closely pressed together, so to speak; we have just seen that this fear was unjustified. The theorem of the theory of ensembles which is necessary and sufficient for demonstrating this result in a rigorous manner is the following: *If on a segment of a straight line we have an infinite number of partial segments (in this particular case, the projections of the spheres of action) whose total length is less than the length of the segment, there exist on that segment an infinite number of points which do not pertain to any of the partial seg-*

ments. This formulation is almost self-evident, and besides, it would be easy to demonstrate it rigorously.

In the case of the plane we shall replace the spheres by circles and the plane perpendicular at a point of the segment by a perpendicular straight line; we can easily prove that, even in the region where the singular points are everywhere dense, there are points at which an infinite number of such lines intersect, on which the density is zero; at these points the logarithmic potential function satisfies Laplace's equation in two variables. If we study in a similar way the function of a complex variable with poles dense in one region, we define an infinite number of straight lines of continuity, intersecting in all directions, the function admitting of derivatives which are continuous on these lines, and the derivative having the same value in all the directions in each of the points of intersection. To express this fact we shall use the word created by Cauchy for designating functions which admit of a derivative independent of the argument of the increment of the variable; these functions may be called monogenic, but they are not analytical, if we reserve for the word "analytical" the very definite meaning which it has possessed since the labors of Weierstrass.

Without lingering on the physical analogies suggested by the existence of planes which do not intersect the spheres of action of the attracting masses, I should like to insist a little upon the nature of the mathematical problems arising out of the existence of these monogenic but not analytical functions. We know that the essential property of analytical functions is that they are determinate in the whole domain of their existence, when their values are given in one portion, however small it may be, of that domain. Is that property a consequence of analyticity—that is to say, of the existence of the Taylor series with radius of convergence differing from zero

—or of monogeneity—that is to say, of the existence of the unique derivative? This question was meaningless as long as it was possible to confound analyticity with monogeneity; on the other hand, it takes a very clear signification as soon as we have succeeded in constructing non-analytical monogenic functions.

I cannot enter to-day into the detail of the deductions which have led to the solution of this problem;[1] here is the result: it is, indeed, monogeneity which is the essential character to which the fundamental property of analytical functions is due; this fundamental property subsists for the non-analytical monogenic functions as soon as we specify clearly the nature of the domains in which these functions are considered. I have proposed to call the domains satisfying these distinct conditions "domains of Cauchy." A domain of Cauchy is obtained by cutting off from a continuous domain domains of exclusion analogous to the spheres of action just mentioned, domains which may be infinite in number, but whose sum can be supposed to be less than any given number (just as the spheres or circles of exclusion just considered, whose radii once chosen we can multiply by any number less than unity, while we are free to increase the upper limit of the density at the same time as we decrease the radii of exclusion).

The series formed by these excluded domains should, evidently, be supposed to be convergent; moreover, we ought to suppose that its convergence is more rapid than that of a determinate series which it is not necessary to write here. Under these conditions, which refer only to the domain and not to the function, every function which in Cauchy's domain

[1] See Emile Borel, "Définition et domaine d'existence des fonctions monogènes uniformes" (*Journal of the International Congress of Mathematicians*, Cambridge, England, 1912); "Les fonctions monogènes non-analytiques" (*Bulletin de la Société Mathématique de France*, 1912).

satisfies the fundamental equation of monogeneity possesses the essential property of the analytical function; we can calculate it throughout its domain of existence by the knowledge of its derivatives at one point (the existence of the first derivative involves the existence of all the derivatives, at least in a certain domain which forms part of the Cauchy domain), and this mode of calculation implies the consequence that, if the monogenic function be zero on an arc however small, it is zero in every point of the domain of Cauchy; two functions, therefore, cannot coincide on an arc without coinciding throughout their domain of existence, in the generalized sense.

I cannot develop the consequences of these results from the point of view of the theory of functions; but I should like, in closing, to submit to you some reflections which they suggest concerning the relations between mathematical and physical continuity.

VIII

MOST of the equations into which we translate the physical phenomena have certain properties of continuity; the solutions vary in a continuous manner, at least during a certain interval, greater or less in length, when the given quantities vary in a continuous manner. Besides, this property is not absolutely general, and it might happen that the theories of the *quanta* of emission or absorption may lead us to give more importance than heretofore to exceptional cases; but to-day I do not wish to enter upon this discussion; I limit myself to the general property, verified in a large number of cases.

When we seek to interpret this property in the theory of the potential and of the monogenic functions, we should expect, if for simplification we confine ourselves to the real functions of a single variable, to find a sort of continuous passage between such of these functions as are analytical in the Weierstrassian sense and those which are entirely discontinuous. Now, this is precisely what does not occur unless we consider non-analytical monogenic functions; as soon as a function ceases to be analytical it no longer possesses any of the essential properties of analytical functions: the discontinuity is sudden. The new monogenic functions permit one to define functions of real variables which might be called quasi-analytical and which constitute in some way a zone of transition between the classical analytical functions and the functions which are not determined by the knowledge of their derivatives in a point. This transitional zone deserves to be studied: it is oftentimes the study of hybrid forms which best teaches us about certain properties of clearly defined species.

We see that the points of contact between molecular physics and mathematics are numerous: I have only been able to point out, in a rapid manner, the most important among them. I am not competent to ask whether the physicists will be able to derive immediate advantage from these analogies; but I am convinced that mathematicians can only gain by investigating them more thoroughly. Mathematical analysis has ever been rejuvenated by contact with nature; it is only because of this permanent contact that it has been able to escape the danger of becoming a pure symbolism, revolving in a circle about itself; thanks to molecular physics, the speculations on discontinuity will assume their full significance, and will develop in a truly fruitful manner. And while it is impossible to foresee the exact applications of these researches, it is not unlikely that the mental habits they foster will not prove useless to those who desire to undertake the task, that cannot long be deferred, of creating an analysis adapted to theoretical researches in the physics of discontinuity.

EMILE BOREL.

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